Understanding the Relationship between Founder CEOs and Firm Performance

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Abstract

We investigate the impact of founder CEOs on their firms' market valuations and operational performances. Since the previous literature has largely ignored the effect of firm performance on founder-CEO status, in this paper we use instrumental variables methods to uncover a causal relationship between founder CEOs and performance. Using the proportion of the firm's founders that are dead and the number of people who founded the company as instruments for founder-CEO status, we find strong evidence that founder-CEO status is endogenous in performance regressions. After instrumenting for founder-CEO status, we identify a positive causal effect of founder CEOs on firm performance which is quantitatively larger than the effect estimated through standard OLS regressions. We also find that founder CEOs are more likely to relinquish the CEO post after periods of either unusually low or unusually high operational performances.

1. Introduction

In this paper, we investigate the impact of founder CEOs on their firms' market valuations and operational performances. Unlike most of the previous literature, we take the endogenous nature of the founder status as CEO seriously. We use two complementary identification strategies. First, we use instrumental variables methods to disentangle the effect of founder CEOs on firm performance from the effect of performance on founder-CEO status. Second, we estimate the effect of past operational and market performance on *changes* in founder-CEO status. Our findings under these two approaches suggest that: (i) founder CEOs appear to increase market valuations and operational performances of their firms, and (ii) founder CEOs are more likely to relinquish the CEO post after periods of either unusually low or unusually high operational performances.

Earlier research on the effects of founder CEOs on market valuations has produced mixed findings. Johnson et al. (1985) analyze the stock price reaction following the unexpected death of senior corporate executives, and find a positive stock price reaction following the sudden death of a corporate founder. Morck, Shleifer, and Vishny (1988) find a negative effect of founding family control, but only for older firms. For the younger firms in their sample, the market value effect of having a member of the founding family as one of the top two executives is positive. Morck, Strangeland, and Yeung (1998) find a negative correlation between heir control in Canadian firms and firm performance. Anderson and Reeb (2003) provide evidence consistent with family firms having higher market valuations and better accounting performance than non-family firms.

Recently, a new wave of research on the topic has arisen, with a focus on refining the evidence from the previous studies. One strand of this new generation of papers focuses on inherited control. The evidence from the US (Pérez-González, 2006) and Denmark (Bennedsen et al., 2007) is consistent with the original findings by Morck et al (1998): inherited control by a family member is bad for firm performance. In contrast, Sraez and Thesmar (2007) find not only that family control improves performance, but also that even heir-controlled

family firms have higher performance in France.

Another strand of this recent literature focuses on the effects of founder control on performance. In research contemporaneous to this paper, Fahlenbrach (2006), Palia et al (2007), and Villalonga and Amit (2006) all find a positive effect of founder CEOs on firm performance. Our paper differs from these mainly due to our identification strategies. We also present some unique findings, especially with respect to the effect of firm performance on founder-CEO turnover. However, a natural process of mutual influence has also produced some ideas and findings that are shared by all these papers. Our main contribution to this literature is our focus on the importance of endogeneity. We believe that this contribution has already had a positive effect on these contemporaneous papers.

In regressions of market valuations and return on assets on founder-CEO status and other controls, we propose two instruments for the founder-CEO status variable. The first is the proportion of the firm's founders who are dead. The second is the number of people who founded the company. Our primary sample consists of data on Fortune 500 firms over the 1992-1999 period, for which we could gather data on the proposed instruments. Using these instruments, we find strong evidence that founder-CEO status is endogenous in performance regressions, which implies that the effect of founder CEOs cannot be correctly estimated using ordinary least squares methods. After instrumenting for founder-CEO status, we find evidence consistent with a positive causal effect of founder CEOs on firm performance.

Using the timing of the events and first-differencing the dependent variable as an alternative identification strategy, we also examine the effect of past extreme performances on the likelihood that founders retain the CEO title. We find that both good and bad past performances increase the probability that founders step down. We conjecture that the effect of good performance on founder-CEO departures might be due either to a "controlled succession" effect (Morck, Shleifer and Vishny, 1989), whereby founders who wish to transfer control to their heirs can accomplish this more easily following good performance, or more simply to the fact that founders leave their companies only when they are in good shape

(Wasserman, 2003).

Our paper is about identification. In particular, our main challenge is to find proper instruments for the founder status as CEO. We provide a number of economic arguments and statistical tests that corroborate our identification strategies. There are sound reasons for our proposed instruments to be correlated with founder control. Indeed, we show that they are. In addition, statistical tests cannot reject our orthogonality restrictions. Thus, a unique combination of economic arguments and statistical tests in supporting our identification strategies is what sets our paper apart from the others in this literature.

We start in section 2 by describing our sample, which we use in section 3 to examine OLS regressions of performance on founder-CEO status. In section 4, we address the endogeneity of founder-CEO status. Section 5 provides further evidence on the causal relationship from performance to founder-CEO status, and section 6 concludes the paper.

2. Data Description

Our primary sample consists of data on publicly traded firms in the 1998 Fortune 500, excluding regulated financial firms and utilities, during 1992-1999 for which data are available on Standard and Poor's ExecuComp (2000). From ExecuComp we obtain the names of the sample firms' CEOs, CEO ownership and tenure as CEO as well as some financial information. We gather the remaining financial information from Compustat and the date of the firm's incorporation from Moody's Industrial Manuals (1999), proxy statements and annual reports for fiscal 1998. Our final sample consists of 2,128 complete firm-years of data for 321 firms during the 1992-1999 time period.

Since ExecuComp does not contain information on whether the CEO is also a founder, for the all firm-years we checked whether the current CEO was one of the firm's founders in a variety of sources consisting of proxy statements, annual reports and the internet.[1] We

[1] When we could find the name of the firm's original founders this procedure was straightforward. However, very few proxies, annual reports or company websites disclosed the names of the original founders. We were

set *founderCEO* in a given year equal to 1 if any source explicitly named the current CEO as a founder or the main executive at the time the company began (including when it was spun-off).[2]

We use both a market-based measure of performance for our sample firms, a proxy for Tobin's Q, as well as an accounting measure, ROA. We define Tobin's Q to be the ratio of the firm's market value to its book value. The firm's market value is calculated as the book value of assets minus the book value of equity plus the market value of equity. We define ROA to be the ratio of net income before extraordinary items and discontinued operations to its book value of assets. Alternatively, we also use EBITDA instead of net income as the numerator for ROA.

We use "average Tobin's Q" as a measure of market valuation (scaled by the book value of assets), and not as a proxy for investment opportunities. *Holding the book value of assets constant*, maximizing Q is equivalent to maximizing the market value of the firm, which is considered the proper objective of the firm by most financial economists. Thus, in order for a Q regression to be properly interpreted as a valuation regression, one must always include the book value of assets as a right-hand side variable.

In Table 1 we present summary statistics concerning select financial variables and CEO characteristics. During our sample period a founder was the CEO at some point for 50 of our sample firms (15.6% of firms). On the whole a founder was the CEO during 11.1% of firm-years.

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most successful conducting searches using the Google search engine.

[2] We assigned a value of zero to the *founderCEO* dummy if the firm was incorporated at least 64 years prior to the current year. The longest period of time a CEO has been working for his firm in our sample is 47 years. Since most firms are founded several years prior to the date of incorporation, this procedure ensures that we check more CEOs than are likely to be founders. All results remain unchanged if we treat these observations as missing.

3. The Empirical Correlation between Founder CEOs and Firm Performance: Ordinary Least Squares Estimates

As a first step in understanding the relationship between founder-CEO status and firm performance, we check whether the retention of the CEO title by the founder is correlated with firm performance in our sample. That is, here we try to replicate some of the findings of the related literature. Our main contributions are in Sections 4 and 5, in which we discuss possible endogeneity problems and causality issues.

Our measures of firm performance are Tobin's Q and ROA. We use two benchmark models for performance throughout this paper. The first one postulates that the variable *founderCEO* might affect performance along with other firm-level characteristics, which are the log of total assets (a proxy for firm size), the log of firm age, a measure of stock return volatility and 2-digit industry dummies (we omit time and firm subscripts, and y is the performance variable):

$$\begin{aligned} y &= b_0 + b_1 \textit{founderCEO} + b_2 \ln(\text{assets}) + b_3 \ln(\text{firm age}) + b_4 \text{volatility} \\ &\quad + \text{industry dummies} + \text{time dummies} + u. \end{aligned} \tag{1}$$

We do not use firm fixed effects in our specification because our main explanatory variable (*founderCEO)* varies little over time for a given firm.[3] To calculate all t-statistics, we use heteroskedasticity-corrected standard errors. In addition, to account for over-time correlation within the same firm, we cluster observations by firm.

In Table 2, we report the results of regression (1) for the two performance measures.

[3]If the explanatory variable changes slowly over time (as *founderCEO* does), firm fixed-effect regressions may fail to detect relationships in the data even when they exist. Using a larger sample, Fahlenbrach (2006) shows evidence of a positive correlation between founder CEOs and performance even with firm fixed effects.

Column I reports the results using $\log Q$ as the performance measure,[4] and Column III reports the results using ROA as the performance measure. *FounderCEO* is significantly positively correlated with both $\log Q$ and ROA at the 1% significance level (the p-values are 0.001 and 0.004, respectively). Of the other three variables, volatility enters with a significant negative sign in both regressions, while firm size and age have significant negative effects only when performance is measured by ROA.

Because it is plausible that *founderCEO* is correlated with CEO characteristics, it is possible that these results reflect a spurious correlation between *founderCEO* and performance that is due to omitted variables. In our second specification, we therefore include several CEO characteristics in an attempt to correct for this problem. In particular, we identified three obvious candidates for which *founderCEO* might be considered a good proxy variable. The first is CEO ownership: it is likely that founders hold a disproportionately large fraction of the firm's equity. It is also reasonable to expect that founders would have long tenures in the firm before leaving the CEO position. Finally, the fraction of the CEO's compensation which is based on equity may be correlated with *founderCEO* because of differing pay-for-performance incentives for founders. Because all three of these variables might also have direct effects on performance, we add them to our original benchmark model to get:[5]

$$\begin{aligned} y \;=\; & b_0 + b_1 \mathit{founderCEO} + b_2 \ln(\text{assets}) + b_3 \ln(\text{firm age}) + b_4 \text{volatility} \\ & + b_5 \text{CEO ownership} + b_6 \text{CEO tenure} + b_7 \text{CEO equity pay} \\ & + \text{industry dummies} + \text{time dummies} \;+\; u. \end{aligned} \tag{2}$$

In Columns II and IV of Table 2, we report the results of regression (2) for the two performance measures. Consistent with omitted variable concerns, we find in both specifications that the coefficients on *founderCEO* are smaller than those in the previous specifications.

[4] We chose a log-linear specification for Q due to the fact that Q can never be negative. Using Q instead of $\log Q$ as the dependent variable might therefore generate fitted values that are outside of the range of Q.

[5] Following Anderson and Reeb (2003), we use the value of CEOs' annual option pay divided by the sum of salary, bonus and option pay to measure a CEO's equity-based pay.

However, *founderCEO* is still significantly positively correlated with $\log Q$ at the 1% significance level (the p-value is exactly 0.01) and with ROA at the 10% significance level (the p-value is 0.056). CEO ownership is also significantly positively related to performance, while CEO equity pay is only significant in the $\log Q$ regression. CEO tenure does not have a statistically significant effect on performance.

Taken at face value, what is the economic significance of these results? It is important to note that because we are using a log-linear specification for Q, the marginal effect of founder CEOs on Q varies positively with the level of Q. In our sample, the average Q is 2.05. If we take the estimated coefficient on *founderCEO* from regression (2) as our estimate of the effect of founder CEOs on Q, our results suggest that a firm with an average Q will experience a drop of 0.37 units in Q whenever its CEO is not also one of its founders. This effect is not trivial, but also not too large: it is about one fourth of the sample standard deviation of Q.

Our OLS estimates of the effects of founder CEOs on the different measures of firm performance are comparable to the ones reported by Anderson and Reeb (2003). Using a different sample-selection procedure and different empirical models from the ones we use in this paper, they find that founder CEOs have a marginal effect on Q of 0.47 units. While our marginal effect for the average firm in our preferred specification is somewhat lower (0.37), our log-linear specification is not directly comparable to theirs, because our estimated marginal effects are not constant. When we re-estimate our preferred model using Q instead of $\log Q$ as the dependent variable, we obtain an estimated marginal effect of 0.52 ($t = 2.30$; p-value $= 0.022$), which is not statistically different from 0.47 at any reasonable significance level. Although the log-linear specification appears to produce more conservative estimates than the linear specification, we continue to use our log-linear specification for Q because the differences are small. In addition, our maximum likelihood approach in the next sections requires that, conditional on the right-hand side variables, the dependent variable should be normally distributed. This assumption may not be unreasonable for $\log Q$, but it is false by

construction for Q.

The similarities between our results and the ones found in Anderson and Reeb (2003) also extend to accounting measures of performance. They find that founder CEOs have a marginal effect on ROA of 3.14 (when ROA is measured using net income, as in this paper), an effect that is somewhat larger than the one we report in Column IV of Table 2 (1.75), but fairly close to the one we report in Column III (2.77). They also use a different proxy for the return on assets based on EBITDA (earnings before interest, tax, depreciation, and amortization) in their regressions. For comparison, we re-estimated our two benchmark models using EBITDA instead of net income as the numerator for ROA. Our estimates for the coefficient on *founderCEO* are 0.031 ($t = 2.35$; p-value = 0.01) and 0.026 ($t = 1.85$; p-value = 0.064) for the first and second model, respectively, which are quite similar to the estimate of 0.035 in Anderson and Reeb (2003).[6]

Overall, it appears that the magnitude of the estimated coefficient on the founder-CEO dummy in a linear performance regression is not very sensitive to the choice of the set of control variables. Furthermore, survivorship biases do not appear to be a major concern in such regressions. Anderson and Reeb's (2003) procedure of choosing firms in the S&P 500 in 1992 and then following them until 1999 introduces a very different type of selection bias than our approach of choosing the firms in the Fortune 500 in 1998 and following them back in time. Nevertheless, the fact that our estimates are virtually identical to theirs suggests that these different types of survivorship biases are not creating a discrepancy between the two sets of findings. Furthermore, our findings are in broad agreement with the ones of Fahlenbrach (2006), Palia et al. (2007), and Villalonga and Amit (2006). Because each of these papers uses different samples and different control variables, we conclude that the positive relationship between the retention of the CEO title by one of the founders and both market and accounting measures of firm performance appears to be fairly robust.

[6] Although our results are similar when we use EBITDA instead of net income to construct ROA, it is easier to detect evidence consistent with the existence of an endogeneity problem using EBITDA. Thus our choice of net income instead of EBITDA is more conservative.

Thus, rather than questioning whether this positive relationship is spurious, the important question, to which we turn next, is how should one interpret this relationship? Should one conclude that the retention of the CEO title by one of the company's founders leads to superior performance? Or is it the other way around, that is, is superior performance a reason for a founder to remain as CEO? Or is it both? To gain further insight into the nature of the relationship between founder CEOs and firm performance, we try to disentangle these different effects in the remainder of the paper.

4. Assessing the Causal Relationships between Founder CEOs and Firm Performance

4.1. Two-Stage Least Squares Estimates

In this section, we use two-stage least squares methods to isolate the effects of founder CEOs on performance from other sources of variation. We first discuss the economic arguments supporting the validity of the two different variables that we use as instruments for *founderCEO*, which are *dead founders* and the *number of founders*, and then we describe the details on the construction of the instruments and their summary statistics.

- *Dead Founders*

The first variable we use as an instrument is a dummy variable that takes the value of 1 if the founder died before the start of our sample period and zero otherwise (if there are multiple founders, we take the average of this variable among all founders). The motivation for this instrument is simple: dead founders cannot be CEOs. However, to be a good instrument *dead founders* must also be uncorrelated with performance except through explanatory variables contained in the second stage regression. We find it unlikely that founders' deaths are caused by performance. The death of a founder should be a fairly exogenous event which will affect

the likelihood that the current CEO is one of the founders but that does not have a plausible direct effect on performance, except when the founder happens to be in control.

A possible caveat is that "dead founders" may be correlated with firm age, which could have direct effects on firm performance. Because we control for firm age in all of our regressions, we believe that this should not be an important concern. It is conceivable that the "dead founder" variable may be correlated with the firm life cycle in a manner that is not captured by the firm age variable, but we have no strong reasons to believe that this should be the case. As is the case with any identification strategy, the results are as good as the identifying assumptions. We believe that ours are not too strong.

- *Number of Founders*

The second variable we use as an instrument is the number of founders of each firm. We believe that this variable also satisfies the conditions necessary for a valid instrument. First, the probability that the current CEO is one of the founders is increasing in the number of founders, although since one founder often plays a more dominant role than the others we expect this correlation to be weaker than in the case of our other instrument.[7] Second, it should be fairly exogenous in our setup. In particular, the number of founders is unlikely to have any direct effect on firm performance years after the founding event.

A possible caveat is that different industries might require different number of founders. However, we found no systematic variation in the number of founders that is explained by industry classifications. Furthermore, all our regressions include industry dummies.

[7] The case of Arrow Electronics illustrates how the number of founders may influence whether or not the current CEO is a founder (see Hoovers 2002, Fortune, January 12, 1981, p. 19 and The New York Times, December 6, 1980, p. 26). In 1968 three friends led a group of investors in acquiring a then obscure company called Arrow Electronics Corporation. After merging it with another company, they used it to found what is now one of the largest distributors of electronic components in the country. One of the partners, Duke Glenn, Jr., was the Chairman and CEO. The other two were Executive Vice-Presidents. In 1980 a hotel fire killed 13 members of Arrow's senior management including the founder/CEO and another founder. The remaining founder, John Waddell, was immediately named acting CEO and remained CEO with only brief interruptions until 1986. Although Waddell's primary responsibilities were in corporate administration and communications before the fire, the crisis led the board to choose him as acting CEO because he was one of the original founders.

A further concern has to do with the quality of the *founderCEO* variable. Arguably, this variable is only an imperfect proxy for a latent "founder control" variable. For example, Villalonga and Amit (2006) report that not only founder CEOs, but also founder chairmen, have positive effects on performance. In principle, one should not attach too much significance to the titles; what is important is whether the founder has any influence on managerial decisions. Thus, we view the *founderCEO* variable simply as a proxy for founder control. The measurement error problem associated with this variable will be attenuated if our instruments are correlated with the latent "true variable." Indeed, "dead founders" and the number of founders are likely to affect "true" founder control, and not only the type of control captured by the *founderCEO* dummy.

4.1.1. Construction of Instruments and Summary Statistics

We collected the data necessary to construct the instruments from a variety of sources using Lexis-Nexis as well as the International Directory of Company Histories (various volumes) and company histories on company websites when available. In order to determine who the founders of the firms in our sample are, we first had to establish what the founding event of the firm in its form in which it appears in 1998 was (since our firms are taken from the 1998 Fortune 500 list). We consider the following four types of events to be founding events: a simple business start-up (e.g. a shop opening), a merger of equals, a spin-off of a division that was not previously a separate company that had been acquired and a major change in ownership, e.g. an LBO, MBO or other acquisition, that leads to a major change in the development of the company. In the case of a merger of equals, we consider the founders of the new company to be the founders of both firms that are merging. In the case of a spin-off we consider the founders to be the founders of the original company, as well as the CEO at the time of the spin-off if he appears important to the development of the company. If a company was acquired and spun-off again, we consider the founders to be the founders of the company pre spin-off. We also generally consider any person to be a founder of the

company who is identified as such in any of our data sources. In some cases our sources also identify important investors in the company or the first CEO who was hired by a founder as founders.

Our procedure was to use the company descriptions in the International Directory of Company Histories and the histories of the companies in Hoover's Company Profile Database, as well as information on the founders of the 1992 Fortune 200 firms in the National Commission on Entrepreneurship's (2001) study on entrepreneurs as a starting point for identifying the founding event, and if possible, the names of the founders. This procedure worked better for firms that were founded recently than for older firms that had undergone several mergers or restructurings. Generally older firms tended to have company histories on their websites that we could use to identify what the firm considers to be its main founding event. Once we identified the founding events, we searched archived stories from the sources Forbes, Fortune and U.S. News on Lexis-Nexis for further information on the founders of the company and information on whether or not the founders died prior to 1992 and the year the founders died. We consider a founder to be alive after 1992 when we could either verify that he was alive after 1992 or we could not find an obituary for the founder and the founder is mentioned in news articles as playing an important role in the company after 1975. When we were unable to find the necessary information on Lexis-Nexis, we searched for the founders using Forbes' Peopletracker and the internet.

Our final data set consists of 580 observations on founders for 321 firms in our sample. Our instruments are a straightforward per-firm average of the dummy indicating whether the founder died prior to 1992 and the per-firm sum of all founders.

Most firms in our sample were founded by simple business start-ups. Approximately 21 firms were founded through mergers of equals and 13 were founded as the result of a spin-off. The average number of founders in our sample is 1.8 with a standard deviation of 1.1 and a maximum of 8 founders. 50% of the firms were founded prior to 1961. This is reflected in the fact that the average proportion of founders who died prior to 1992 is 70.3% with a

standard deviation of 43.5%.

None of the results that follow are sensitive to how our instruments are constructed. Variations in the way we code the data lead to virtually the same results. For example, the results are qualitatively similar if we restrict the sample only to firms that were founded as start-ups.

4.1.2. Results

Here we describe the results of our 2SLS regressions. Table 3 reports the outcomes of the first-stage regressions of *founderCEO* on our two instruments and the other controls from the models (1) and (2). From Table 3 we see that both proposed instruments are significantly correlated with *founderCEO*: consistent with intuition *dead founders* enters the regressions negatively with robust (clustered by firm) t-statistics of -5.51 and -4.31 in columns I and II, respectively, while the *number of founders* enters them positively with robust (clustered by firm) t-statistics of 1.64 and 1.95 in columns I and II, respectively. The partial R^2's of the excluded instruments are 13% and 8% for each equation respectively. An F-test that both instruments are excluded from the first stage is easily rejected at any traditional significance level (see Table 3 for the F-statistics). Thus, we conclude that our instruments not only move the $founderCEO$ variable in the predicted direction, but also that our specifications do not appear to suffer from problems associated with "weak instruments."

In Table 4, we report the results of the second-stage regression using model (1) for the two performance measures. From Columns I and II we see that *founderCEO* is significantly positively related to $\log Q$ at the 1% significance level (the p-values are always lower than 0.001). Similarly, in Columns III and IV we see that *founderCEO* is significantly positively correlated with ROA at the 1% significance level (the p-values are always lower than 0.004). These results suggest a *causal* positive effect of founder CEOs on performance. Under the assumption that our instruments are valid, our results suggest that firms with founders as their CEOs appear to perform better than others on average.

We can test for the null that our instruments are valid by means of a test of over-identifying restrictions. Because our model is over-identified (we have two instruments), under the null that the instruments are valid (i.e. they are uncorrelated with the error term and correctly excluded from the estimated equation), the J-statistic is distributed as chi-squared with one degree of freedom. As we report in the bottom of Table 4, in all four cases the J-statistics are fairly small, with p-values ranging from 51% to 77%. Thus, one cannot reject the null that our instruments are valid, under any reasonable confidence level. In fact, the J-statistics are so low that we never reject the null even if we take an extremely conservative confidence level, such as 50% (rather than the more traditional conservative level of 10%).

Perhaps surprisingly, the estimated coefficients on *founderCEO* are *larger* when we use 2SLS instead of OLS. Furthermore, the differences between the 2SLS and the OLS results are statistically significant. At the bottom of Table 4, we report the differences between the 2SLS and OLS estimates of the effect of founder CEOs on performance, along with their t-statistics, which are computed using the method in Hausman's (1978) specification tests. We find that all differences are statistically different from zero at the 1% significance level. Thus, under the maintained assumption that the instruments are valid (which appears quite reasonable given the low J-statistics), we find the OLS effects are significantly smaller than the 2SLS effects. We cannot reject that there is significant endogeneity in the one-equation procedures that try to estimate the effect of founder CEOs on market and accounting measures of performance. Thus, OLS procedures that ignore the endogeneity of founder-CEO status in performance regressions, such as the ones in the previous section, can be potentially misleading because they generate inconsistent estimates of the economically relevant parameter of interest.

If we take the estimated coefficient on *founderCEO* from the 2SLS regression of model (2) as our estimate of the effect of founder CEOs on Q, our results imply that a firm with average Q will experience a drop of 3 units in its Q if its CEO is not also one of its founders. This effect is almost 10 times larger than the one estimated by OLS. However, it must be kept

in mind that instrumental variable techniques capture local effects; that is, the regressions capture the value effect of the variation in founder-CEO status that is explained by the variation in our instruments. A firm that would like to have a founder CEO, but cannot because founders are either dead or generally in short supply, is likely to suffer more than a firm that chooses not to have a founder CEO, even when there are many founders available.[8]

4.2. Endogenous Dummy Variable Model

One noticeable feature of our 2SLS procedure is that, although *founderCEO* is binary, in the first-stage regression we ignore the discrete nature of this variable. Two-stage least squares consistency of the second stage does not hinge on getting the functional form right in the first stage, thus one does not necessarily have to use a discrete dependent variable model for a dummy endogenous variable (see Angrist and Krueger, 2001). However, 2SLS lead to biased estimates in finite samples and it is not known how misspecification in the first stage may affect this bias. Therefore, in this section we jointly estimate a system of equations in which we explicitly account for the binary nature of *founderCEO*.

We formulate the following model:

$$y = b_0 + b_1 founderCEO + \mathbf{X}\boldsymbol{\beta} + u_1 \tag{3}$$

$$I = \mathbf{Z}\boldsymbol{\alpha} + u_2 \tag{4}$$

$$founderCEO = \begin{cases} 1 \text{ if } I \geq I^* \\ 0 \text{ if } I < I^* \end{cases} \tag{5}$$

where $u_1 \backsim N(0, \sigma^2)$, $u_2 \backsim N(0,1)$ and $corr(u_1, u_2) = \rho$. Equation (3) is the same one we have estimated by single-equation procedures: the vector of controls $\mathbf{X}$ varies depending on whether we are estimating model (1) or (2). Equation (4) models the determinants of founders retaining the CEO title. I is an unobservable variable that measures the aggregation

[8] Using a different instrument, Fahlenbrach (2006) finds a marginal effect of founder CEOs of 1.45 Q units.

of forces that favor retaining a founder as the CEO. The determinants of I are the observable variables $\mathbf{Z}$ that one believes should affect the likelihood that a founder keeps the CEO title plus an unobserved error u_2. Equation (5) is the function that models the decision to keep a founder as the CEO: founders retain the CEO title if and only if I is above an (unobservable to the econometrician) threshold level I^*.

The model above is one example of an *endogenous dummy variable model* (Heckman, 1978). Notice that the correlation between the errors of the two equations allows many interpretations. For example, if unusually good performance implies that the founder is more likely to retain the CEO title, we should expect ρ to be positive. We should also expect ρ to be positive if there is an omitted variable that affects performance and *founderCEO* in the same direction.

We can estimate the above system of equations by maximum likelihood to get estimates of all relevant parameters.[9] We use *dead founders* and *number of founders* as our main determinants of I in the second equation. The first equation replicates the models (1) or (2).

Table 5 reports the results for both models and both performance measures. As before, the direct effect of *founderCEO* on performance is always positive and significant at all conventional significance levels. It is also true that all estimated coefficients are larger than their OLS counterparts, which is consistent with the results from the previous subsection. However, the magnitudes of these effects are no longer so large: for example, the estimates from column II suggest that a firm with average Q will experience a drop of 0.78 units in its Q when its CEO is not one of its founders. This effect is about twice as large as the one estimated by OLS but much lower than our 2SLS estimate. We conclude that the magnitudes of our 2SLS estimates are not robust to changes in model specification. However, the evidence that there is a direct positive effect of founder CEOs on performance remains overwhelming.

At the bottom of Table 5 we report our estimates of the correlation between the errors of

[9] This model is identical to the standard *average treatment effects* model encountered in the program evaluation literature (see Maddala, 1983, for many examples).

the two equations, ρ. Consistent with the hypothesis that good performance might reduce the likelihood that a founder retains the CEO title, we find that ρ is always negative in all four columns. The estimated correlation between the errors of the two equations also appears quite sizeable: it is approximately -0.4 in the Q models. Wald tests of the null that this correlation is zero yield p-values lower than 5% in all four cases, and lower than 1% in three of the four cases. Since testing the null that the correlation coefficient ρ is zero is an explicit test of the exogeneity of founder-CEO status (under the assumption that the model is otherwise correctly specified), we reject the null that the two equations in (3) and (4) are independent in all of our specifications.[10]

We summarize the results in this section as follows. We confirm the previous findings that firms that keep one of their founders as their CEOs perform better than the ones that do not, but the extremely large estimated partial effects of founder CEOs on performance that we found in the previous subsection appear to be a feature of the 2SLS specification. Most importantly, we find strong evidence that founder-CEO status is not independent of performance, and that once one factors out the direct effect of founder CEOs on performance, the remaining correlation between firm performance and the likelihood that a founder retains the CEO title is negative, as evidenced by the negative value that we find for ρ. This is consistent with different interpretations and we discuss the reasonableness of some of these interpretations in more detail in the next section.

5. Past Performance and Founder-CEO Departures

In this section, we try to address the opposite question to the one in the previous sections: What is the effect of firm performance on founder-CEO status? In the absence of instruments for performance, we use a different identification strategy. We assess how well past performances, both good and bad, help predict future changes in command in which a founder

[10] If we construct our proxy for ROA using EBITDA instead of net income, we always reject the null of independence of equations at the 1% significance level.

CEO steps out. Thus, we use the timing of events as an identification strategy. There are two main limitations of this procedure. First, predictive power does not imply causation, especially when variables reflect the behavior of forward-looking agents. Thus, we expect market measures of performance to be more plagued by endogeneity problems in predictive regressions than accounting measures, because the latter tend to be less influenced by the expectation of future events. Second, and perhaps most importantly, our tests in the previous sections detected a *contemporaneous* effect of performance on founder-CEO status. To the extent that performance exhibits some persistence, our approach in this section should be able to shed some light on the causes of this effect. However, one cannot fully capture this effect without accounting for its strictly contemporaneous component, for which identification by means of timing is not feasible.

Our empirical strategy is as follows. When a CEO who is not a founder is replaced, this typically (though not necessarily) implies that the new CEO is also a non-founder. Therefore, turnover data in firms which are not initially run by founders are not useful for our purposes. Accordingly, we restrict our sample to firms that were run by one of their founders in any year in our sample. We then generate an indicator variable called *stepout* that takes the value of 1 in the firm-years in which a founder-CEO steps out and 0 otherwise. For each firm that has experienced a change of command in this restricted sample, we leave out all observations in the years after the one in which the founder has relinquished the CEO title. This sample selection procedure severely reduces the number of usable observations. We therefore expanded our sample to increase the number of changes in command for founder CEOs. To do this we used Forbes executive compensation surveys (Forbes, 1992-1999), which identify whether or not the CEOs of the Forbes 800 are founders. We first matched ExecuComp to the Forbes 800 firms to identify further instances of firms whose founders were CEOs. We then tracked these additional firms in the Forbes compensation surveys until 2001 to identify when the founder no longer held the CEO title. By this procedure we were able to expand our sample of founder-CEO departures from 23 to 50. We also identified

535 firm-years in which *stepout* is equal to zero. We obtain performance measures and other controls, such as total assets, volatility, CEO ownership, and CEO tenure, from ExecuComp for the additional firm-years.

There are reasons to believe that founder-CEO departures should be differentially affected by either good or bad past performance. It is well known that bad performance triggers CEO turnover (e.g., Warner, Watts and Wruck, 1988; Weisbach, 1988). On the other hand, after good performance, CEOs may be more likely to be able to choose their successors (Morck, Shleifer and Vishny, 1989). Founders in particular may value the ability to control succession, for it allows them to transfer control to their heirs. Thus, the controlled succession hypothesis predicts that founder CEOs will step out after some period of consistently good performance. Additionally, founder-CEO's success in achieving critical milestones could be a critical determinant of founder-CEO exit (Wasserman, 2003). Thus, the relationship between performance and the likelihood of founder-CEO departures is potentially non-monotonic. In order to jointly test some of these hypotheses, we have to impose some empirical specifications that allow for the possibility of a non-monotonic relationship between the dependent and independent variables.

A simple and intuitive procedure that is well suited for our purposes is as follows. We create a dummy variable called *highQ* that equals 1 for very high values of lagged Q and is zero otherwise. We consider Q to be high if it is in the top quartile of the full sample Q distribution for that given year. Similarly, we create a dummy variable called *lowQ* that equals 1 whenever Q is in the bottom quartile of the full sample Q distribution for that given year, and is zero otherwise. The variables *highROA* and *lowROA* are defined in an analogous way. Because we want to see the effects of *persistent* past performance on CEO turnover, we use the averages of one- and two-year lagged Q and ROA to construct our measures of extreme performance.[11] Theses variables partially capture the relative nature of performance, that is, how the firm is performing in relation to others.[12]

[11] We chose two years because using three or more years would severely restrict our sample size.

[12] Changing the comparison group appears to have only minimal effects on these variables. For example,

Table 6 presents the results of Probits estimating the likelihood of a founder-CEO stepping out as a function of *highQ* and *lowQ* and year dummies, and also as a function of *highROA* and *lowROA* and year dummies. As one can see from column I, we find that the estimated coefficients on *highQ* and *lowQ* are both positive but not significant. From column II, however, we find that both *highROA* and *lowROA* help predict future changes in which a founder-CEO steps out.[13]

The finding that past poor performance as measured by *ROA* increases the likelihood that a founder-CEO will leave the firm is at odds with the hypothesis that founders are inexorably entrenched. This result is not surprising, given the well-documented evidence on the disciplining role of CEO dismissals (Warner, Watts and Wruck, 1988; Weisbach, 1988). What our evidence adds to this literature is the confirmation that founders are not immune to this disciplining device.

More interesting is the finding that founder CEOs departures are also more likely after periods of good accounting performance. In our sample, this effect is unique to founder departures. For example, if we replicate regression II in Table 6 using all CEO departures rather than founder-CEO departures, we find that although poor ROA performance predicts CEO departures (the probit coefficient is 0.275, with a p-value of 7%), the coefficient on *highROA* is negative and not statistically significant. We have replicated all regressions we report in this session using either total or non-founder-CEO departures, and we found no evidence of good past performance increasing the likelihood of CEO departures (we do not report the detailed results to economize on space).

Here we briefly consider some robustness checks. First, our choice of cutoff (25%) to define both high and low performance is not important. Choosing any cutoff in the range of 10% to 35% always leads to estimated parameters that are significant at least at the 10%

in earlier versions of this paper we have used the full sample (1992-1999) as the comparison group, and the results were almost exactly identical.

[13] The lack of predictive power of Q is not surprising. In fact, Hermalin and Weisbach (1998) claim that this is exactly what theory predicts: accounting measures of performance reflect the characteristics of current managers, while stock market based measures of performance should also reflect the expectation of future management changes.

level in the ROA specification. As expected, as the cutoff approaches 50% the effects become much weaker and eventually not significant. For cutoffs lower than 10%, the standard errors tend to go up, which is consistent with the intuition that the precision of our estimates should decrease as the number of firms that are considered to be performing extremely well or extremely badly decreases.

We also experimented with including other controls in our specifications (not reported in the tables). Firm-level controls do not have significant effects on the probability of a founder-CEO departure. For example, firm size (proxied by the log of total assets) and volatility have no significant effect on founder-CEO departures. Their inclusion also does not change the significance of the results reported in Table 6. On the other hand, CEO tenure and CEO age do have significant effects on founder-CEO departures, but again they have minimal impact on the estimated effects of both bad and good performance on CEO departures.

As a final robustness check, we also tried a more flexible specification that is capable of capturing richer non-monotonic relationships between performance and the probability of CEO departures. For each performance measure, we created two new variables: we interact both the high and the low performance dummies with the average of one- and two-year lagged performance. We estimate Probits using both the dummies and the interaction terms. This specification allows us to capture the additional effect that, for example, performance has on founder-CEO departures *conditional on performance being high*. It also allow us to better explore the continuous nature of the underlying performance variables. We report the results of these extended specifications in columns III and IV of Table 6. We see that the significance of the effects of both the low and high performance dummies is not affected by the inclusion of the interaction terms. Furthermore, these interaction terms appear to have no additional predictive power. This suggests that our original simpler specification is capturing most of the effects of performance on the likelihood of founder-CEO departures.

One possible reason for founder CEOs to leave after good performance is that, if they want

to retire when rich, they should be more willing to retire early following good performance. A related but somewhat darker story is that founder CEOs might be better informed than other shareholders and may choose to leave the firm and sell their shares when performance is unusually high. Thus, founder CEOs may leave their firms to "cash in" before the market valuation of their shares deteriorates. In both cases "wealth effects" are important, i.e. founder CEOs want to leave their firms exactly when their firm-related wealth is high.

In Table 7 we examine the importance of wealth effects.[14] If wealth effects are important determinants of founder-CEO departures, one should expect that founder CEOs with more firm-related wealth leave more often. In column I, we see that the opposite holds: founder CEOs with higher ownership stakes are less likely to leave the firm. Of course, ownership may affect the likelihood of departure for reasons that are not related to CEO wealth. For example, CEOs with more ownership might be more entrenched and thus less likely to be forced to leave. Strictly speaking, wealth effects should play a role only when CEOs are departing after good performance. To test this hypothesis more directly, we create a dummy variable that takes the value of 1 if founder-CEO ownership is "high" and 0 otherwise. We define high ownership as an above-average equity stake (the average ownership of founder CEOs in our sample is approximately 5%). We then interact the high ownership dummy with both *highROA* and *lowROA*. If founder CEOs are departing after periods of good performance due to wealth effects, then we should expect that the sensitivity of departure to good performance should be higher when they have more ownership. Thus, we expect the interaction between *highROA* and *highOWNERSHIP* to have a positive effect on the probability of CEO departure. From column II, we see that this interaction term actually enters with a negative sign and is not significantly different from zero. Overall, we find no support for the hypothesis that wealth effects are responsible for the positive correlation between high performance and founder-CEO departures in our sample.

[14] We only report results using ROA. As before, the regressions using Q suggest similar results, but they are never significant.

6. Conclusions

In this paper, we provide strong evidence that founder-CEO status is influenced by firm performance. Using methods based on instrumental variables, we find evidence of endogeneity in standard OLS regressions of firm performance on founder-CEO status. This result is not sensitive to model specification, to alternative measures of performance, or to econometric procedures. After factoring out the effect of performance on founder-CEO status, we identify a positive causal effect of founder CEOs on firm performance, an effect that is quantitatively larger than the one estimated through standard OLS regressions.

We also estimate the probability of founder-CEO departures as a (potentially non-monotonic) function of past performance. Contrary to the hypothesis advanced in much of the previous literature, that founder-CEOs will retain control when performance is high, we find that past superior accounting performance increases the likelihood that founder-CEOs will step out.

Our paper has implications for the growing literature on family firms. In particular, the positive effect of founders on performance suggests that the higher performance of family firms that is found in many studies could be driven primarily by firms in which the current CEO is a founder.

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Table 1
Summary Statistics

Sample consists of 321 publicly traded, non-regulated firms from the 1998 Fortune 500 that were available on ExecuComp (2000) during the years 1992-1999. Most financial and CEO data are from ExecuComp (2000). Firm age is collected from Moody's Manuals (1999), proxy statements and 10-Ks for fiscal 1998. Founder data are from a variety of sources consisting of proxy statements, annual reports and the internet. Our proxy for Tobin's Q is = (book value of assets-book value of equity + market value of equity)/book value of assets. ROA = net income before extraordinary items and discontinued operations/book value of assets. $FounderCEO$ is equal to one if the CEO is a founder of the company. CEO ownership is defined as the ratio of the number of shares owned by the CEO after adjusting for stock splits to total shares outstanding. CEO tenure is the number of years since the CEO was appointed CEO. CEO equity-based pay is the value of annual option pay divided by the sum of salary, bonus and annual option pay. $Volatility$ is the Black-Scholes volatility as reported in ExecuComp. Firm age is the number of years since the firm's first date of incorporation.

Variable	Mean	St. Dev.	Min.	Max.	No. Obs.
Q	2.05	1.40	0.83	19.16	2128
$logQ$	0.59	0.46	−0.18	2.95	2128
ROA	5.68	5.75	−48.19	48.15	2128
$FounderCEO$	0.11	0.31	0.00	1.00	2128
CEO ownership	0.02	0.05	0.00	0.44	2128
CEO tenure	7.36	7.24	0.00	47.00	2128
CEO equity-based pay	0.47	0.29	0.00	1.00	2128
Volatility	0.29	0.11	0.12	1.05	2128
Firm assets (log)	8.72	1.07	5.78	12.91	2128
Firm age (log)	3.82	0.89	0.00	4.98	2128

Table 2
OLS Regressions of Firm Performance on Founder-CEO Status

This table reports results of regressing firm performance (measured alternatively by $logQ$ and ROA) on founder-CEO status. Columns I and II report results using $logQ$ as the performance measure. Columns III and IV report results using ROA. For each performance measure, we estimate the empirical model in equations (1) and (2) in the text using OLS. All data are described in Table 1. The estimation period is 1992-1999. All regressions include year effects and 2-digit SIC industry dummies. The estimations correct the error structure for heteroskedasdicity and within-firm correlation. Robust t-statistics are in parentheses.

	Dependent Variable			
	$logQ$		ROA	
Indep. Variables	(I)	(II)	(III)	(IV)
$FounderCEO$	0.242***	0.185***	2.770***	1.751*
	(3.31)	(2.58)	(2.91)	(1.91)
ln(assets)	−0.025	−0.026	−0.519**	−0.470**
	(−1.12)	(−1.18)	(−2.19)	(−2.03)
ln(age)	−0.037	−0.038	−0.471	−0.479*
	(−1.29)	(−1.33)	(−1.59)	(−1.66)
$Volatility$	−0.858***	−0.932***	−16.217***	−16.988***
	(−4.62)	(−5.13)	(−6.96)	(−7.50)
CEO ownership	.	1.327***	.	20.085***
		(2.81)		(3.24)
CEO tenure	.	−0.002	.	−0.012
		(−0.68)		(−0.45)
CEO equity pay	.	0.159***	.	0.955
		(3.01)		(1.570)
Observations	2128	2128	2128	2128
Adj-R^2	0.30	0.32	0.19	0.20

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 3
2SLS Regressions of Firm Performance on Founder-CEO Status: First Stage

This table reports the first-stage of the two-stage least squares regressions relating firm performance to founder-CEO status for both models (1) and (2) in the text. We instrument $founderCEO$ using *dead founders* and *number of founders*. *Dead founders* is the average of an indicator variable that takes the value of 1 if a given founder is dead as of 1992 and zero otherwise. *Number of founders* is the total number of founders for each firm. Details on the construction of the instruments are provided in the text. All other data is described in Table 1. The estimation period is 1992-1999. All regressions include year effects and 2-digit SIC industry dummies. Partial $R2$'s and F statistics (constructed with a robust variance-covariance matrix) of the excluded instruments are reported at the bottom of the table. Robust (clustered by firm) t-statistics are in parentheses.

	Dependent Variable: *FounderCEO*	
Indep. Variables	(I)	(II)
Dead founders	−0.293***	−0.190***
	(−5.51)	(−4.31)
Number of founders	0.022*	0.022**
	(1.64)	(1.95)
ln(*assets*)	−0.007	0.006
	(−0.59)	(0.54)
ln(*age*)	−0.050***	−0.061***
	(−2.92)	(−3.81)
Volatility	0.183	0.249*
	(1.13)	(1.67)
CEO ownership	.	1.299***
		(3.32)
CEO tenure	.	0.012***
		(5.48)
CEO equity pay	.	0.030
		(0.96)
Partial R^2 of instruments	0.14	0.08
$F(2, 320)$ (Test of excluded instruments)	18.14	11.36
Observations	2128	2128
Adj-R^2	0.40	0.52

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 4
2SLS Regressions of Firm Performance on Founder-CEO Status: Second Stage

This table reports results of regressing firm performance (measured alternatively by $logQ$ and ROA) on $FounderCEO$ status. $FounderCEO$ is instrumented with $dead\ founders$ and $number\ of\ founders$. The first stage regressions are reported in Table 3. Columns I and II report results using $logQ$ as the performance measure. Columns III and IV report results using ROA. For each performance measure, we estimate the empirical model in equations (1) and (2) in the text. All data are described in Table 1 and the Appendix. The estimation period is 1992-1999. All regressions include year effects and 2-digit SIC industry dummies. The estimations correct the error structure for heteroskedasdicity and within-firm correlation. For each regression, we report a Hansen J-test statistic of over-identifying restrictions alongside with its p-value (using robust standard erros). The bottom row of this Table reports an estimate of the difference between the IV and the OLS coefficients on the $founderCEO$ variable, computed using a Hausman (1978) specification test. Robust t-statistics are in parentheses.

	Dependent Variable			
	$logQ$		ROA	
Indep. Variables	(I)	(II)	(III)	(IV)
$FounderCEO$	1.096***	1.485***	8.998***	10.843***
	(4.33)	(3.63)	(3.81)	(2.95)
ln($assets$)	−0.100	−0.028	−0.410*	−0.490**
	(−0.41)	(−1.08)	(−1.73)	(−1.96)
ln(age)	0.052	0.087**	0.177	0.396
	(1.52)	(2.05)	(0.53)	(1.01)
$Volatility$	−1.196***	−1.443***	−18.687***	−20.558***
	(−4.53)	(−4.44)	(−6.86)	(−6.70)
CEO ownership	.	−0.547	.	6.981
		(−0.56)		(0.77)
CEO tenure	.	−0.020***	.	−0.141**
		(−3.21)		(−2.39)
CEO equity pay	.	0.100	.	0.543
		(1.52)		(0.78)
Observations	2128	2128	2128	2128
J-statistic	0.084	0.350	0.280	0.427
p-value	0.773	0.554	0.597	0.514
Diff. IV - OLS	0.854***	1.300***	6.230***	9.093***
	(8.883)	(7.948)	(5.345)	(4.861)

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 5
Firm Performance and Founder-CEO Status: Results From Endogenous Dummy Variable Model

This table reports results of regressing firm performance (measured alternatively by $logQ$ and ROA) on founder-CEO status. $FounderCEO$ is instrumented with $dead\ founders$ and $number\ of\ founders$. The estimation method takes the discrete nature of $founderCEO$ explicitly into account. Columns I and II report results using $logQ$ as the performance measure. Columns III and IV report results using ROA. For each performance measure, we estimate the empirical model in equations (3) to (5) in the text. We use the same control variables as in Tables 2 to 4. All data are described in Table 1 or in the text. The estimation period is 1992-1999. All regressions include year effects and 2-digit SIC industry dummies. The estimations correct the error structure for heteroskedasdicity and within-firm correlation. The bottom row reports an estimate of the correlation between the error terms of equations (3) and (4) in the text. The P-value is from a Wald test of the independence of equations. Robust t-statistics are in parentheses.

	Dependent Variable			
	$logQ$		ROA	
Indep. Variables	(I)	(II)	(III)	(IV)
$FounderCEO$	0.436***	0.383***	3.579***	2.514***
	(5.42)	(4.53)	(3.76)	(2.78)
ln($assets$)	−0.022	−0.023	−0.507**	−0.460**
	(−1.01)	(−1.08)	(−2.17)	(−2.02)
ln(age)	−0.028	−0.028	−0.430	−0.439
	(−1.03)	(−1.04)	(−1.50)	(−1.57)
$Volatility$	−0.936***	−1.012***	−16.521***	−17.265***
	(−5.19)	(−5.73)	(−7.28)	(−7.83)
CEO ownership	.	1.293***	.	19.907***
		(2.84)		(3.27)
CEO tenure	.	−0.002	.	−0.013
		(−0.83)		(−0.50)
CEO equity pay	.	0.150***	.	0.919
		(2.89)		(1.54)
Observations	2128	2128	2128	2128
Correlation estimate	−0.395***	−0.397***	−0.121***	−0.112**
P-value ind. eqs.	0.0002	0.0003	0.0052	0.0123

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 6
Probit Estimates of Founder-CEO Succession

In this table we examine the effect of lagged performance on the likelihood that a founder retains the CEO title. We restrict our sample to firms that were run by one of their founders in any year in our sample. The dependent variable (*Stepout*) is a dummy which takes the value of 1 in the firm-years in which a founder-CEO steps out and is 0 otherwise. For each firm that has experienced a change of command in this restricted sample, we leave out all observations in the years after the one in which the founder has relinquished the CEO title. Columns I and III report results using $logQ$ as the performance measure, and Columns II and IV report results using ROA. The variable $highQ$ is a dummy which is equal to one if the average of the first two lags of Q is at the top quartile of the Q distribution in any firm-year, and is zero otherwise. The variable $lowQ$ is a dummy which is equal to one if the average of the first two lags of Q is at the bottom quartile of the Q distribution in any firm-year, and is zero otherwise. The variables $highROA$ and $lowROA$ are constructed in the same way. $LaggedQ$ and $laggedROA$ are the averages of the first two lags of each performance variable. All data are described in Table 1. The estimation period is 1992-1999. All regressions include year effects. The estimations correct the error structure for heteroskedasdicity using the Huber-White estimator. Robust z-statistics are in parentheses.

	Dependent Variable: *Stepout*			
Indep. Variables	(I)	(II)	(III)	(IV)
$HighQ$	0.117 (0.67)	.	0.594 (1.48)	.
$LowQ$	0.311 (1.22)	.	0.426 (1.56)	.
$HighROA$	.	0.472* (2.48)	.	0.862** (2.32)
$LowROA$	.	0.723*** (3.38)	.	0.719*** (3.21)
$LaggedQ * HighQ$	.	.	−0.180 (−1.32)	.
$LaggedQ * LowQ$	.	.	−1.892 (-1.13)	.
$LaggedROA * HighROA$	.	.	.	−0.014 (−1.19)
$LaggedROA * LowROA$	.	.	.	−0.002 (−0.21)
Observations	459	466	459	466

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.

Table 7
Probit Estimates of Founder-CEO Succession with Ownership Controls

In this table we examine the effect of ownership controls on the likelihood that a founder retains the CEO title. We add lagged ownership and two interaction terms between ownership and performance to the specification in column II of Table 6. We define an indicator variable called $highOWNERSHIP$ which takes the value of 1 if lagged ownership is above 0.05. The estimation period is 1992-1999. All regressions include year effects. The estimations correct the error structure for heteroskedasdicity. Robust z-statistics are in parentheses.

	Dependent Variable: *Stepout*	
Indep. Variables	(I)	(II)
$HighROA$	0.514***	0.562***
	(2.62)	(2.59)
$LowROA$	0.663***	0.554**
	(3.02)	(2.19)
Lagged Ownership	−2.308*	−2.243
	(−1.89)	(−1.34)
$HighOWNERSHIP * highROA$	.	−0.151
		(−0.44)
$HighOWNERSHIP * lowROA$	.	0.356
		(0.94)
Observations	461	461

***,**,* indicate statistical significance at 1%, 5%, and 10% (two-tail) test levels, respectively.